
Notes and Insights

Revisiting *Street Corner Society*

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Almost 50 years after its publication, Street Corner Society was the focus of new scholarly attention in Reframing Organizational Culture and a special issue of the Journal of Contemporary Ethnology. Whyte comments on the issues raised regarding the intellectual background of the book and his relations with the community studied. He also discusses the new style of criticism embodied in critical epistemology in its relations with social science.

KEY WORDS: field relations; positivism; critical epistemology; social science.

Almost half a century after its publication in 1943, *Street Corner Society* has suddenly become a new focus of scholarly attention. *Reframing Organizational Culture* (Frost *et al.*, 1991) devotes a major part to "Exploring an Exemplar of Organizational Culture Research." That section begins with a long excerpt from my 1955 appendix to *Street Corner Society* (hereafter also referred to as SCS), follows with 4 critiques of the book by behavioral scientists (Michael Owen Jones, Alan Bryman, Patricia Riley, and John M. Jermier), and concludes with my "Comments for the SCS Critics." The April 1992 issue of the *Journal of Contemporary Ethnography* is entirely devoted to *Street Corner Society*.

Following an introduction by the editors, the issue leads off with a paper by W. A. Marianne Boelen, who went back to "Cornerville" on several occasions 30–45 years after I had left it in 1940, and interviewed some people who knew me and other residents who did not. She raises questions regarding the validity of my data and my interpretation of those data. She also charges me with ethical transgressions involving my relations with Cornerville and with Doc, my principal guide to the district. That is followed

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by my rejoinder, an essay by a former corner boy who worked with me on the study, Angelo Ralph Orlandella (Sam Franco in *SCS*), and 3 papers by behavioral scientists (Arthur J. Vidich, Laurel Richardson, and Norman K. Denzin).

With the essays by the 7 behavioral scientists, I did not have to defend my character or the reputation of the book, because they all accepted *Street Corner Society* as "a sociological classic," in those words or others to that effect. They did, however, raise questions that throw an interesting light upon the way standards of criticism have changed over the past half century, particularly with the recent popularity of critical epistemology and deconstructionism. Although the criticisms are directed at my work, they have a more general significance, applying also to generations of social scientists dedicated to systematic observation and detailed analysis of "real-world" behavior. These social scientists are not content simply with telling stories that can be countered by other storytellers, but are searching beyond the stories for the foundations of human social behavior.

Since Orlandella and I have answered Boelen's charges in some detail in the *Journal of Contemporary Ethnography*, here I will only discuss points she raises that have some general relevance for evaluating field studies for today and tomorrow. In what follows I will be dealing primarily with two references. I will refer to *Reframing Organizational Culture* as *ROC* and the *Journal of Contemporary Ethnography* as *JCE*.

I will first describe the history of *Street Corner Society* from the first edition (1943) to the third (1981). Then I will discuss its intellectual background. I will then summarize the critical points on field methodology and professional ethics raised by Boelen and by one or more of the behavioral science critics. Finally, I will present my own position on epistemology, focusing on the presumed clash between positivism and current trends in critical epistemology.

ON THE UNNATURAL HISTORY OF A BOOK

I wanted to publish a book that would attract readers outside the academic community. I first tried a commercial publisher, who responded with a flattering rejection letter. The University of Chicago Press then agreed to publish, but the business manager required a subsidy of \$1300 because *Street Corner Society* was not expected to sell many copies.

The 1943 edition stirred hardly a ripple of interest among my colleagues. It was not reviewed in *American Sociological Review* and got only a lukewarm reception in *American Journal of Sociology*, the reviewer telling readers that here was another good study of a slum district. Royalties were

very modest in 1944 and practically nothing in 1945. With the World War II veterans returning to college, supported by the GI bill, some professors decided to require their students to read monographs rather than just textbooks. Sales picked up in 1946, but by the early 1950s they were down to a trickle.

In this period, I was teaching a seminar on field research methods, and I had looked in vain for frank and detailed accounts by participant observers on how they had done their studies. What they had written on methodology read to me like "this is the way I would have done it if I had known at the beginning what I knew at the end." Although participant observation was often practiced by social anthropologists, there were no anthropological courses in field methodology being taught at Cornell. The orientation of students before they set out for the field generally consisted of little more than a chat with their principal professors.

In writing up my experiences in the SCS research, I resolved to tell the story in detail, dealing with the early fumbling efforts and confusions in trying to find what my study was about and noting some of the mistakes I had made. I even acknowledged an ethical violation (intervening in an argument in a club in order to improve my relations with a racketeer I was studying) and a crime I committed (voting 4 times in a congressional election). I did this not only to point out pitfalls to avoid but also to encourage students to believe that sometimes even serious mistakes might not doom a study.

The 1955 edition, with the methodological appendix, had a dramatic impact on sales, and the paperback edition in the 1960s helped keep the book alive. By the mid-1960s I began to hear people referring to *Street Corner Society* as "a sociological classic." The value some of my colleagues found in the appendix may have encouraged other behavioral scientists to write up their own field experiences frankly and in detail, so that today there is far more of such material available than there was in the early 1950s.

For my retirement in 1980, the ILR dean authorized me to invite for the presentations 7 individuals who had been my students or with whom I had worked on research. The first of these was Angelo Ralph Orlandella (Sam Franco). He gave such an interesting and eloquent talk that several in the audience told me that it should be printed. That gave me an idea for the 3rd edition, adding a new appendix on his "The Whyte Impact on an Underdog." Then it occurred to me to write some notes on the history of the book, and especially on the problems I had with getting it accepted as a doctoral thesis without footnotes or the obligatory first chapter, reviewing the literature. The critics in *Reframing Organizational Culture* were naturally puzzled about how those typical features were avoided, since they had not read the 1981 edition, which contains the answers. (By the time of my Ph.D. thesis examination, the University of Chicago Press had ac-

cepted *SCS* for publication. The Sociology Department then had a requirement—long since abandoned—that all Ph.D. theses had to be printed at the author's expense. The \$1300 subsidy to the Press had exhausted our savings, and I did not want to lay out more money to print a review of the slum literature—most of which I disagreed with—to be bound with the book in the university library. With the mediation of Everett C. Hughes between me and Louis Wirth and Herbert Blumer, I managed to get around that requirement by getting accepted for publication two articles based on reviews of the literature.)

For the 1981 edition, it occurred to me that it would interest readers to know that Cornerville was the North End of Boston, and more than 40 years after I had left the district, I could see no harm in doing so. Doc had died in 1967. Since I had admired him and since many readers see him as the “hero” in *SCS*, I identified him in this edition. According to Boelen, his sons, born after I left Cornerville, objected to this identification.

THE INTELLECTUAL BACKGROUND OF *SCS*

Jermier believes that “*SCS* is marked by the powerful influence of Chicago School epistemology” (*ROC*: 227). Boelen makes the same point, with a negative twist: that I distorted my interpretations in order to bring them in line with the Chicago School of Sociology.

As I pointed out in the 1981 edition (which none of the seven behavioral science critics had read), I had completed a first draft of *SCS* before I entered graduate work at the University of Chicago, and subsequent condensing and rewriting did not change in any way my analysis of the North End data. While I was in the field, 1936–1940, I thought of myself as a student of social anthropology. I had read widely in that field, under the guidance of Conrad M. Arensberg. At that time, I had not read any of the University of Chicago Urban studies. In the thesis examination, I had to resist the efforts of Louis Wirth and Herbert Blumer to put my argument into the social disorganizational framework then popular in Chicago and elsewhere.

ON THE RESEARCHER'S RELATIONS TO THOSE STUDIED

Boelen (*JCE*: 33–34) asks of me,

Did he commit an ethical cardinal sin by not taking his manuscript back to the field and checking the data and contents with the subjects?

This "ethical cardinal sin" is Boelen's creation. At the time of my study, I had never heard of such an obligation. Today some sociologists and social anthropologists are advocating some kind of feedback to the field, but still I know of no code of professional ethics in sociology or anthropology that makes such a requirement. Assuming that I had tried to carry out Boelen's principle, how would I have gone about it? How does one feed back the data and contents of such a study to a community of 20,000 people—or even to the parts of the community I focused on?

Should I have fed back my findings on the social ranking and leadership pattern to the Nortons, as a group? When I once asked them who their leader was, they stated they were all equal. To reveal to them that behaviorally they were not equal would have embarrassed Doc and upset his followers.

Before I left, Doc read the manuscript I took to Chicago, and we had long conversations over his suggestions and criticism. I also had innumerable feedback discussions with Sam Franco. The settlement house director read my study of the Nortons before hiring Doc to direct a storefront recreation center. Beyond *SCS*, in much of my work I have made it a point to discuss my findings and interpretations with participants in the field.

Note that Boelen deals with field relations only in terms of the researcher's presumed obligations to those studied. She does not consider the right of the researcher to publish conclusions and interpretations as he or she sees them. How to balance our obligations to those we study against our rights as authors to publish our findings is a complex question that cannot be answered by dealing only with our obligations to informants. In *Learning from the Field* (1984), I have discussed some aspects of that question.

Did I exploit Doc? Boelen reports that his sons think I did, that I should have shared *SCS* royalties with him. I acknowledge that I gained more from our relationship than he did, but at the time I tried to reciprocate as best I could (*JCE*: 61).

Assuming that Doc himself thought I owed him something further, Richardson (*JCE*: 116) offers this hypothesis:

Whyte saw Doc as co-researcher, whose interpretations were intermingled with Whyte's. Ultimately, however, Whyte single-authored *SCS*: Whyte received the fame and "fortune" associated with the book. The fortune probably seemed immense to Doc, who was habitually underemployed.

The problem with this hypothesis is that it puts Doc on "hold" and me on "fast forward." In 1943, when *SCS* was published, my savings had been exhausted by the subsidy to the press, and I earned nothing for the

year in which I was recovering from polio, beginning in June 1943. The first edition yielded in royalties little more than the amount of our subsidy. It was not until after the 1955 edition was published that SCS began to bring in substantial royalties. And the last time I saw him in 1953, Doc still greeted me as a friend.

In my Cornerville period, Doc was indeed "habitually underemployed," but the war boom beginning in 1942 got him a job where he was doing well until the postwar cutback came, and he was laid off. Somewhat later, he got a job with a major electronics company. There he moved up into management. On my last visit with him (December 1953), he was assistant supervisor of production planning. When he died in 1967, he was manager of production planning, a key middle management position.

Beyond my personal experience, what general conclusions can we draw about relations between researchers and his key informants? Should they be paid? If so, how much would be fair? And how should fairness be determined? It seems to me impossible to lay down any universal rule for such questions. For the researcher to promise money for informant interviews seems to me to inject a mutually calculating element into a relationship which works best when both parties agree voluntarily to collaborate. In some cases, it may be impossible to avoid payment commitments, but such commitments could substantially increase the costs of research, so as to rule out some otherwise mutually desirable projects.

Should a contingency payment be promised—a share in book royalties? That seems highly unrealistic for sociological or anthropological monographs. It is only rarely that such a monograph gains substantial sales—and, in my case, 13 years after the book's initial publication.

I guided my involvement with Doc in terms of the principle of interpersonal reciprocity. When we were working together, I tried to be helpful to him, and Doc seemed satisfied with the relationship. He may later have come to the conclusion that I exploited him, as his sons now believe.

Following the principle of interpersonal reciprocity provides no guarantee that the relationship will seem equitable to a key informant years later—or to his sons.

If interpersonal reciprocity provides no guarantee of maintaining good relations between researchers and principle informants and collaborators, can we find another basis for building that relationship?

Along with some of my colleagues, I have become convinced that *participatory action research* (PAR) provides one important means of bridging the gap between professional researchers and members of the organization we study. This is a methodology in which researchers invite some members of the organization studied to participate with them in all phases of the

process, from research design through data gathering and analysis, and on to the practical application of research findings. PAR is still unfamiliar to most behavioral scientists, but it has been practiced (usually under other labels) since at least the 1960s (Whyte, 1989, 1990, 1991; Whyte *et al.*, 1989; Harkavy and Puckett, 1991; Greenwood *et al.*, 1993).

In terms of the issues discussed here, PAR research has a dual advantage. In field relations, it enables us to go beyond interpersonal reciprocity in linking key informants and professional social researchers. As members of the community or organization studied become committed to the hoped-for action outcomes of the research process, they become less concerned with what they are getting personally for what they are doing *with* the researcher. This also can relieve researchers of the uncertainty and anxiety over whether we have done enough for them personally in return for what they have done for us.

PAR also helps us to deal with one of the concerns of critical epistemologists: opening up ways in which at least some members of the organization studied add their own insider voices to those of the outsiders. This can enrich the data gathering and analysis process and also increase the level of acceptance of the research report within the community or organization studied.

The potentialities and limitations of PAR are currently being explored. It would not have been possible for me in the late 1930s because I was striving to follow the then current Harvard norm emphasizing a commitment to "pure science," without any involvement in social action. Also I did not have a secure position in an organization that would have given me a chance to organize a PAR project. Implementation of the strategy is most effective when the social researcher is a member of a continuing organization capable of developing a long-term relationship. The lone researcher is in a poor position to provide the necessary follow-through.

The PAR strategy can only be applied effectively in a limited number of situations. Where it is possible, it offers opportunities to improve researcher field relations, to strengthen the research process, and to achieve practical results.

ON THE POSTFOUNDATIONAL CRITIQUE

When I agreed to answer the Boelen attack, I assumed that the 3 behavioral scientists would conclude that my 3½ years of field work, backed up by voluminous notes typed shortly after the events or interviews, would be a more accurate guide to the realities of Cornerville in the late 1930s than the recollections of selected informants 30–45 years later. None of

the 3 takes a position on that issue. Vidich (*JCE*: 80) simply states that "Readers may draw their own conclusions about the issues raised in these essays," but then he goes on with tributes to the continuing value of *SCS* for social theory and practice in urban slum areas. Richardson and Denzin do not deal with the issue because for them the nature of the critical game has changed since I did the study. Richardson (*JCE*: 103–104) states that she writes about *SCS*

now in a radically different context from that in which it was produced. Some refer to the present intellectual context as "postfoundational." The core of this post-foundational climate is *doubt* that any discourse has a privileged place, any text an authoritative "corner" on the truth.

Denzin (*JCE*: 130) calls me a "positivist-social realist" and states that

Today, social realism is under attack. It is now seen as but one narrative strategy for telling stories about the world out there. (*JCE*: 126)

Riley (*ROC*: 218) argues along the same lines. Interpreting the argument of Clifford Geertz, she writes

cultural descriptions, filtered through the ethnographer, are actually second or third order fictions . . . there is no culture or organization "out there" to be accurately represented by observers.

In *Works and Lives* (1988), Geertz discusses the problems faced by students of culture, as indicated in his subtitle: *The Anthropologist as Author*. He sees social anthropologists confronting an intellectual crisis:

They are also harassed by grave inner uncertainties, amounting almost to a sort of epistemological hypochondria, concerning how one can know that anything one says about other forms of life is as a matter of fact so. (1988:71)

After examining the works of some of the most eminent social anthropologists (Levi-Strauss, Evans-Pritchard, Malinowski, and Benedict), he abandons any hope of establishing scientific conclusions. He speaks rather of "rendering your account credible through rendering your person so" (1988:79). He adds,

Ethnography takes, obliquely in the 1920's and 1930's more and more openly today, a rather introspective turn. To be a customary "I-witness," one must, so it seems, first become a convincing "I."

Ethnological writing thus comes to depend on persuasion of the reader. But, he adds,

Who is now to be persuaded? Africanists or Africans? Americanists or American Indians? Japanologists or Japanese? And of what: Factual accuracy? Theoretical sweep? Imaginative grasp? It is easy enough to answer, "All of the above." It is not quite so easy to produce a text that thus responds. (1988:133)

ON THE POSTFOUNDATIONAL FRAMEWORK AND SOCIAL SCIENCE

When I began my SCS research, I wanted to contribute to building a science of society—and I have not yet given up that commitment. I based my own framework on a basic distinction between the *objective* (what is out there to be observed) and the *subjective* (how the researcher or others interpret the observed phenomena). I assumed that I should concentrate on the objective, trying in so far as possible to base my interpretations on what I observed and what I was told by informants I had found to be perceptive and accurate observers.

Reflecting on “postfoundational” ethnology, I have come to recognize that the objective–subjective distinction is not as clear as I once thought. Though my study of the social structure of street corner gangs was based primarily upon direct observation, researchers cannot observe everything, and if we tried, we would end up with miscellaneous data, which would yield no intelligible pattern. We seek to observe behavior that is significant to our research purposes. Selection therefore depends upon some implicit or explicit theory—a process that is in large part subjective. But the choice is not random. If we specify our theoretical assumptions and the research methods we use, others can utilize the same assumptions and methods to either verify or challenge our conclusions.

Following the theoretical framework first proposed by Eliot D. Chapple and Conrad M. Arensberg (1940), I concentrated attention on observing and roughly quantifying frequencies and duration of *interactions* among members of street corner gangs and on observing the initiation of *changes in group activities*. (Such an approach had not been used before by sociologists and is still uncommon among sociologists and social anthropologists today.) For the determination of informal group leadership, I relied on the critical distinction between *pair events* (interactions between two people) and *set events* (interactions among three or more people). In observing pair events, I found I could not always determine who was the more influential. In observing set events, the pattern became clear.

In the case of the Nortons, I determined that Doc was the leader through the following types of observations. Before he arrived at his corner, I would see small groups of 2 or 3 conversing. When Doc arrived, the small groups would dissolve and a larger group would form around him. When another member spoke to the group but then noticed that Doc was not listening, he would stop and then try again to get Doc’s attention. Doc often, but not always, was the one to suggest a change in group activity. When another member made a proposal for action not endorsed by Doc,

no activity change followed. Only if Doc made or approved the proposal did I observe a change in group activity. The observational methods I used in the late 1930s for determining informal group structures can be checked today by any researcher who wishes to observe an informal group over an extended period of time.

Regarding the theoretical significance of such structural observations, I reject Riley's (*ROC*: 219) statement that my conclusions on the sociology of bowling and the relationship between changes in the interaction pattern and mental health

have proved more heuristic than others but they should be viewed as a particular conversation, bound in time and space by the rules governing their production.

That statement takes me back to the methodological arguments I encountered in graduate work at the University of Chicago in the early 1940s. In that era, the great debate was between the case study and statistics. Proponents of the case study argued that it yielded "understanding," whereas proponents of statistics claimed that was the only way to science. We students liked to promote a debate between Herbert Blumer (case study) and Samuel Stauffer (statistics), and the same debate was once projected on the national scene between Blumer and George Lundberg. That one became so heated that, at the end, they shook hands so as to give the misleading impression that no hard feelings were involved.

I enjoyed those debates and yet I was unhappy with the way the issues were framed. On the statistics side, the implicit assumption was that we were dealing with social surveys, then as now the main tool for sociologists using quantitative methods. Since the 1950s, I have used surveys in various studies, but in the 1940s I had no use for surveys: I wanted to quantify observations of behavior.

Contrary to Riley's statement, I argue that the case study can discover uniformities that can be checked further in other case studies either experimentally and/or quantitatively. Furthermore, it can yield insights that lead to theoretical advances by the author or by others.

Working with Muzafer Sherif, O. J. Harvey (1953) carried out an experiment on boys groups to check the relation between their social rankings and sports performance. His findings, paralleling mine, hardly confirm that relationship for every case or circumstance, but at least they demonstrate the possibility of checking case study findings experimentally.

The relationship between marked changes in interaction patterns and mental health can be checked in clinical practice to determine whether this framework could be useful in psychotherapy.

Anthropologist Scudder Mekeel (1943) discovered a close parallel between my thesis, "The Social Role of the Settlement House" (Whyte, 1941),

and the relations between American Indians and officials of the Bureau of Indian Affairs. He found that he only had to change "corner boys" to "Indians" and "settlement house workers" to "B.I.A. Officials," and everything else fitted his case as well as mine. A few years after my North End study, Herbert Gans (1962) found the same pattern of relations between settlement house workers and corner boys in the neighboring West End.

In theoretical development, I have built my own conceptual framework over the years on some of my North End observations (1991), and George Homans (1950) used my analysis of the Norton Street gang in developing his own framework.

IN CONCLUSION

When I began my SCS project, I took it for granted that I should aim to contribute to scientific knowledge. Today many behavioral scientists seem to believe this is an impossible objective. But then I ask myself why so many distinguished scholars take such a defeatist attitude.

I think it is because they are focusing on types of problems that cannot have scientific answers. For example, Geertz and the anthropologists whose works he is studying are concerned with studies of the culture of a tribe or community. Culture has many definitions. In its most inclusive definition, it includes kinship structures and other organizational structures, commonly held myths, beliefs, and attitudes, widely shared practices, rituals and ceremonials, common patterns of interaction and activities, ways of making a living, tools and technologies used, etc. The anthropologist assumes that these elements are not randomly distributed and tries to discover some pattern in their relations.

To make sense out of any presumed pattern of relations among so many different elements requires the researcher to go far beyond simple reporting and description. Success at this task calls for imagination and creativity—highly subjective mental processes. The resulting publication may or may not be persuasive to particular readers, but there is no way in which it can be put to a scientific test.

That does not mean that social anthropological interpretations of a given culture are useless. A good cultural study can provide useful guidance for understanding and communicating with members of that culture. That is not the same as scientific proof—but humans would take very few actions if they only responded to scientifically tested propositions.

If as researchers we are seeking generalizations that can be put to scientific tests, then we must focus on certain elements within culture that

can be directly or indirectly observed and measured. That is what I did in studies of street corner gangs. I cannot claim to have made any comprehensive interpretation of the total culture of Cornerville. Women's roles and family life and the role of the church are hardly dealt with. In effect, I abandoned the goal of a comprehensive study to focus on areas in which I had substantial systematic data: corner gangs and their relations to the rackets and political organizations. The methods I used and the conclusions I have drawn can be built on and improved upon by students of community organization today.

While I reject the standards of critical epistemology, I grant that they may have served a useful purpose in the postcolonial era by inviting outside observers to question their own assumptions of a given culture and to seek the views of members of that culture. But that purpose is not served by another outsider, years later, going into that culture to get selected informants to tell their stories. We may agree that no outsider can really know a given culture fully—but then we must ask whether any insider can fully know his or her culture. In emphasizing the advantages of insider knowledge, let us not forget that an outsider can make important contributions—as Alexis de Tocqueville did in his landmark studies of America decades ago.

The contrast to my own views is most sharply posed by Jermier and Denzin. Jermier (*ROC*: 233) calls me a positivist and states that

Critical epistemologists insist that truth lies in ever deeper levels of subjective reflection and disclosure and that science serves most when it serves least.

Following this view, we are left with an argument over whether my “truth” is better than your “truth.”

Denzin begins his essay by recognizing *SCS* as a “sociological classic,” but then ends his critique on this negative note:

As the 20th century is now in its last decade, it is appropriate to ask if we any longer want this kind of social science. Do we want the kind of classic sociology that Whyte produced and Boelen, in her own negative way, endorses?

What is the alternative approach proposed by the critical epistemologists? If, as Denzin states, what he calls “social realism” is now seen as “but one narrative strategy for telling stories about the world out there,” then the critic can only depend on a judgment of the persuasive power of the author. Scientific arguments are thus transformed into literary criticism. We are then left with standards of judgment that shift with the changing trends of literary criticism.

For the future development of the behavioral sciences, the Denzin position brings us to a dead end. I believe critical epistemology will come

to be seen as a passing fad, and that behavioral scientists who have succumbed to that lure will return to the pursuit of scientific knowledge.

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